PRESIDENTIAL ADDRESS

ORIGINS OF AUSTRALIAN GEOLOGY

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Synopsis

One hundred years ago, when the Linnean Society of New South Wales began, geology in this country was a ' colonial ' science—its base of authority still lay largely in Europe. For almost a century, geology in Australia had been dominated by concepts originating in Europe and transported, more or less uncritically, to a land being explored. European precedent is seen as having exerted, in many cases, a counter-productive influence on geological progress here in the years before 1875.

At the start of our centennial session the Macleay Lecturer, Sir Rutherford Robertson, reminded us of the Society's history, of its achievements and of its aspirations (Robertson, 1974). He spoke of a loyalty to the Linnean tradition and how, especially through the *Proceedings* and the work of our Linnean Macleay Fellows, the Linnean Society of New South Wales has contributed to the cause of natural science in Australia.

Recalling that Linnean tradition I am struck by your sense of history in choosing a geologist to preside at this time. Carl Linnaeus [1707–1778] in his first essay of the *Systema Naturae* (1735) placed Regnum Lapideum first among the equal kingdoms of nature. Perhaps our successors will see fit to remember Regnum Vegetabile in 2074. For the present, however, it is my privilege to speak for the stones and I thank you for the opportunity of doing so.

This historic occasion arouses thoughts of time and, being no prophet, I am constrained to turn retrospectively. Sir Rutherford has tidied the last one hundred years ; I propose to cast further for some of the earlier sources of Australian geology and to examine how geology, a European creation, fared in its transfer to a distant land.

The year 1875 is an interesting point at which to pause for review. In Adelaide the third Australian university was about to open its doors. Ralph Tate [1840–1901], foundation professor of natural science there, joined colleagues in Sydney (Archibald Liversidge [1846–1927], one of our first members) and Melbourne (Frederick M'Coy [1817–1899]), teaching aspects of geological science in a context of general educational rather than professional training. Something of the organization of science at the time is evident in Branagan's (1972*b*) recent survey. By 1875 the colonial governments of New South Wales and Queensland had followed the lead of Victoria and moved to establish geological surveys that continue to function. The sorry record of capricious parsimony shown by all the colonial governments towards geological surveys in the period before 1875, however, is best passed in silence. Only the first Geological Survey of Victoria (1852–1869), directed by A. R. C. Selwyn [1824–1902], was given any real opportunity to demonstrate its worth. Few now seem to be aware of what Australian geology lost when that survey was disbanded in 1869. Selwyn's contributions place him in the first rank of our geological pioneers, yet how many remembered the sesquicentenary of his birth ?

I believe it is time we gave more attention to the rise of Australian geology. The past record is still not seen by many geologists as a source of useful or even interesting experience. To them that record is one merely of the out-of-date and superseded. In fairness they have had few guides. There are, of course, useful contributions to the history of particular subject or geographical areas as, for instance, the presidential addresses of Drs Charles Anderson (1933) and Ida Brown (1946) to this Society. But I can recall only two major historical reviews of Australian geology, those of Tate (1894) and Andrews (1942). Unfortunately, the usefulness of Andrews's study is limited by its poor documentation and his evident failure to examine many of the early primary sources. To underline the failure of scholarship in the twentieth century may I point to the last bibliography of Australian geology—that of Etheridge and Jack (1881).

Tate's history remains essential to anyone seeking a way to the beginnings of geological activity in this country. His concern was achievement in discovery rather than intellectual influences. Tate saw his science moving splendidly along a fine broad highway. There is nothing in Tate about wrong turnings, about blind alleys or those lost in them. " No prejudices or scholastic disputations have retarded our progress, for those who have aided in the work were disciples in the modern school of geology " (Tate, 1894, p. 3). The more I delve in the record of Australian geology the more extraordinary I find that claim to be. It seems to me that most of the disputations of European geology found their way here to be compounded with others of local origin.

A Science of the Earth

The 1780's for us were significant years ; they saw not only the foundation of Sydney but also the publication of statements by Abraham Gottlob Werner [1749–1817] and James Hutton [1726–1797] concerning the materials, processes and history of the earth. These men were to exert profound influence on geology, indeed they are accounted founders of the science. For a recent detailed analysis of Werner's work in geology the reader may refer to Wagenbreth (1967) and other papers in that *Gedenkschrift*. Ospovat (1971) supplies an edited translation of Werner's treatise of 1787. James Hutton is treated succinctly by Eyles (1972) who provides a select bibliography. Hutton's *Theory of the Earth* was issued by the Royal Society of Edinburgh in the year of Sydney's birth ; an extended statement, in two volumes, followed in 1795.

The charges of irreligion and wild speculation hurled against Hutton remind one of the cries raised against Darwin some seventy years later. The comparison has even greater interest in that both Hutton's and Darwin's theories came to achieve after long and painful debate the status almost of received opinion. Werner, a popular teacher and effective synthesizer of information, made an immediate impact.

To Hutton more than to any other belongs the credit for first emphasizing the vast span of geological time. From his demonstration of the significance of unconformities geologists gained an essential tool for unravelling earth-history. Hutton also saw that, with time, processes such as those of erosion presently operative could be efficient causes of profound changes in the earth's surface. This model that called not only for operation in the past of the same causes as act at present but also for action throughout at similar levels of intensity received from the philosopher W. Whewell [1794–1866] the name *uniformitarianism*. It is a special case of *actualism* (cf. Hooykaas, 1970), the general term attached to schemes that depend on the operation of known or actual causes though not necessarily at present levels of intensity.

Roughly actualistic models long antedate Hutton but most tend to be strongly non-uniformitarian. That of G. L. L. Buffon [1707–1788] issued in 1749 in his *Histoire & Théorie de la Terre* offers a useful example. Buffon relied on observable processes but postulated their action in the past at levels sufficient to cause sudden and violent revolutions or catastrophes. Catastrophic action is explicit in the scheme outlined by P. S. Pallas [1741–1811] of St. Petersburgh, one of the first great naturalist-travellers, in his *Observations sur la formation des montagnes* of 1771 (Mather and Mason, 1939, pp. 123–5). There Pallas adopted a three-fold chronology of mountains (Primitive or Primary, Secondary and Tertiary) devised earlier in Italy and Germany. The oldest and greatest (Primitive) mountains, according to Pallas, formed chains with granite the oldest of all rocks as typical core material. Younger mountains were lesser structures and consisted of bedded rocks. The most recent mountains that Pallas recognized in Asia he called Tertiary ; these he attributed to the action of violent floods of water displaced by great volcanic eruptions in the South Seas.

Werner's scheme contains borrowings from Pallas and indeed from a wide range of sources. From these sources Werner produced a synthesis that was not even actualist but to his contemporaries had the great merit of being at once practical and essentially conservative. The oldest mountains stood highest and had granite cores but the Primitive, indeed all, crystalline rocks he argued had formed not from a molten state but by precipitation from an early ocean chemically different from that of the present. For this reason his model acquired the nickname *neptunian* and his followers *neptunists.* By contrast, Hutton urged that heat was a profoundly important geological agent and interpreted crystalline rocks as derived by cooling from melts—hence the label *plutonist* or *volcanist* attached to his ideas and his supporters. These names that in fact refer to limited aspects only of the two systems came to be identified by many with the respective wholes.

By his own claim Werner eschewed speculation. The emerging term *geology* he rejected in favour of his invention *geognosy*—a statement of factual knowledge. As befitted a teacher in a mining school (at Freiberg in Saxony), Werner's approach was pragmatic and his classificatory systems generally easy to apply. Minerals were sorted, for instance, according to their gross external characters. Mountains and their constituents he had by the 1790's grouped (Wagenbreth, 1967) in terms of the old three-fold arrangement modified by insertion of a Transition class between the first and second terms the latter of which he called Flötz (stratified).*

According to Werner, sea-level had diminished as time passed and this occasioned deposition of younger rocks at progressively lower levels on the flanks of Primitive mountains. As he postulated no operations of uplift and folding in earth-history, the observed altitude of a rock ought to offer a clue as to its age—a notion that doubtless helped the sale of travelling barometers. Werner's system of stratigraphy was exclusively lithological. He argued further that any particular formation of, say, limestone or sandstone should have a world-wide extent. Stratigraphic correlation ought thus to be possible by matching rock-types.

The period 1790–1820 is termed the Heroic Age of Geology in Zittel (1901), which remains one of the more judicious accounts of the history of geology. It is popularly represented as a time dominated by conflict between the disciples of Hutton and those of Werner. In fact, Hutton himself founded no school and apart from signs of an increasing acceptance of heat and fusion as geological agents one does not find much evidence in Europe of the spread of Huttonian ideas during the Heroic Age. But then whereas all neptunists might be identified as Wernerians, far from all volcanists subscribed to Huttonian doctrine. I get the impression that the practical advances in European geology during this period came mainly from two sources, namely, from Wernerians testing their master's propositions in the field and from that numerous and scattered band, rather

* This Flötz of Werner embraces the succession of European strata from the Coal Measures to the Chalk.

strong in France, that stood apart. The observations of volcanic regions by F. H. A. von Humboldt [1769–1859] and Leopold von Buch [1774–1853], both of them distinguished former students at Freiberg, led to their early defection from the ranks of strict neptunists, though each persisted with other Wernerian methods (e.g. Humboldt, 1823). Within Werner's lifetime the labours of such as the Abbé R. J. Haüy [1743–1822] in Paris had taken mineralogy far from the simple Wernerian system on its course to the purely physical science of modern times. One must add, however, that for long the term *mineralogist* accommodated many whose interests lay in the collection and natural history of minerals. In Australia the term was often enough to be equated with prospector.

In the field of stratigraphy, too, a method was emerging that was to effect profound modification of Wernerian practice. By the late 1790's William Smith [1769–1839], an English engineer, had discovered that order in stratified sequences of rocks was reflected not only in the spatial, depositional relations of beds but also in their fossil contents, so that fossils afforded a means of correlating strata (Cox, 1942 ; Eyles, 1969). J.L.N.F. (Georges) Cuvier [1769–1832] and Alexandre Brongniart [1770–1847] appear to have discovered something of the same principle, independently. Smith is not even mentioned in the revised edition of their account of the Paris Basin (Cuvier and Brongniart, 1822) issued after the great map of the strata of England and Wales (Eyles and Eyles, 1938) had helped to make Smith's discovery known. Despite their careful work on fossils the Frenchmen betray a tendency to emphasize lithological characters in correlating formations. We see much the same in Brongniart's (1829) later classification of terrains. By then in England Smith was gaining his recognition. Conybeare and Phillips (1822) give an early exposition based on Smith's method and one in which the Wernerian stratigraphic method, as distinct from Wernerian terminology, is specifically rejected.

Strict neptunian ideas were in retreat by the 1820's but the growing acceptance of igneous action came most commonly in notional contexts that harked back to Buffon. Catastrophism in some form had a place in most eighteenth century systems, not least, of course, in that of Werner. Cuvier's vigorous promotion of catastrophism in the *Discours Préliminaire* to his researches on vertebrate fossils published first in 1812 ensured its survival during his lifetime. Catastrophism in England turned to a preoccupation with Noah's Deluge. The biblical flood became the last great catastrophic event in earth-history and Diluvial Geology, the study of physical evidences of the Flood, arose through the efforts of the Rev. William Buckland [1784–1856] and others like-minded.

Uniformitarianism eventually found an able and devoted exponent in Charles Lyell [1795–1875]. His *Principles of Geology* (1830–33), despite its seemingly contentious message, was remarkably influential from the start. Lyellian uniformitarianism may never have been universally accepted by geologists but there is no denying its role in determining much of the later course of the science (Bailey, 1962 ; Wilson, 1972).

Lyell (1833) also outlined a subdivision of Tertiary successions and Tertiary time using the terms Eocene, Miocene, Pliocene, still employed. Of the fifteen terms now taken to constitute the Phanerozoic time-scale, only Carboniferous, Jurassic and Cretaceous antedate the year 1833. By 1841 all the others with the particular exceptions of Ordovician and Oligocene had been defined. Definition does not, of course, imply acceptance but the record is surely a measure of the great activity in European geology during those eight years. It was a time when the so-called British (William Smith's) method of stratigraphy and Lyellian doctrine spread across Europe. Even in the German regions where Wernerian geognosy had been most firmly entrenched the impact of the new influences was no less remarkable for being delayed.

Towards an Australian Geology

Pastures New

Before considering the context in which geology began in Australia, it may be useful to notice how and when the science took hold in another new land. Commentators on the history of North American geology (Merrill, 1924 ; Hazen, 1974 ; Ospovat, 1967) all agree on assigning its beginnings there to the very period when Hutton and Werner were active.

Geology went to an established community in eastern North America, one much after the English provincial style of the time. The devotees of geology were amateurs drawn mainly from the learned professions—teachers, lawyers, physicians and surgeons, and clergymen. Those with a university education most likely had been instructed in classics and mathematics and not, as a rule, in the observation of nature, though it is impressive to note a letter of 1799 (Cohen, 1950, p. 117) claiming that mineralogy had become a favourite branch of study among the young men at Harvard. Of the British universities only those in Scotland could then have matched that (Ritchie, 1952 ; Scott, 1966). There is, indeed, a geological chair at Cambridge founded in 1724 but no instruction in the subject was offered there until the appointment of Adam Sedgwick [1785–1873] in 1818. Mineralogy began at Cambridge a few years earlier through the enthusiasm of Dr. E. D. Clarke [1769–1822]. Oxford was not long in following its rival. Graduates, many of them clergymen, appear among the 'learned' geologists of Australia but not until the cruder aspects of frontier existence had begun to disappear.

At the time of Hutton and Werner, Australia was almost as unknown geographically as it was geologically. Had Australia repeated the pattern of earlier exercises in European colonisation there might have been from the outset a dominating commercial interest. From ancient times Europeans entertained great hopes of riches, mineral and otherwise, in new lands. The fascination of Africa was old ; South America had yielded treasures to Spain and Portugal. The first emigrants to Virginia in 1607 had lively expectations of finding gold and precious stones (Wright, 1949, pp. 31–32), as did Dampier coming to New Holland in 1699 (Dampier, 1703, p. 138). We find them in 1785 pressed upon the British government as a reason for colonizing New Holland (Barton, 1889, p. 430). But considerations of social rather than commercial advantage prevailed. The greater need was to find a remote place to lose an increasing number of convicts. In such a colony discoveries of mineral treasure might be highly disturbing. As late as 1844 we find expression of fears of dire consequences, were reports of gold discoveries to become public (Jervis, 1944, p. 394). Indeed for almost the first forty years of settlement no private exploitation of minerals was permitted (cf. Branagan, 1972*a*).

All the infant colony at Sydney required from the earth in 1788, apart from safe anchorage and a supply of fresh water, was material for shelter. Geology enters at its humblest level, that of usefulness. Sandstone and clay occurred locally in abundance ; only limestone for mortar was lacking. The first geological observations reported by a resident come from Arthur Phillip [1738–1814], first governor of New South Wales, and relate to these materials. His remarks (Vallance and Branagan, 1968, p. 265) are observational and practical, of a type not unexpected of an intelligent layman. Few would think of Phillip as a geologist or even as a naturalist yet he represents the first of a long line of colonial officials who contributed to our geological knowledge. Most of them in the early years at least were serving or former naval or military officers. From their ranks came the first explorers and surveyors (Spate *in* Feeken and Feeken, 1970, pp. 4–32). Many demonstrate in their journals a geological interest to the extent of a practical familiarity with earth-materials ; some, for instance T. L. Mitchell [1792–1855] and Charles Sturt [1795–1869], possessed a rather more sophisticated knowledge of the subject. There must be few parts of the world more liberally endowed with topographical features named in honour of prominent European geologists than Australia—names like Buckland, Cuvier, Dolomieu, Faujas, Fitton, Haüy, Hutton, Lonsdale, Lyell, Murchison, Owen, Sedgwick and Sowerby. Not one of these worthies visited Australia, yet each was sufficient of a hero to some explorer to be thought worthy of remembrance.

Observations of the transit of Venus in 1769 began a fashion for scientific exploration. That event was the reason for Pallas's first excursion in Siberia and, of course, had relevance to Captain Cook's *Endeavour* voyage during which he discovered the coast of New South Wales. The achievements of those expeditions need no emphasis ; their very success encouraged emulation by other European nations. The French, in particular, showed great interest and activity. Between 1788 and 1840 no fewer than eight French naval/scientific expeditions visited Australia ; in that time there had been calls too from Russian, Spanish and American parties as well as several British expeditions. Most were accompanied by trained observers, equipped to consider nature in Australia in terms of current scientific thought.

Those from Europe brought with them the experience of a longer tradition of scientific and technical education than was to be found in England. The Mining Academy at Freiberg has a continuous history from the year 1765. Not long after that date mining schools were active in Russia, in the Austrian Empire and in France ; the earliest such institution in Britain opened in 1851. Nowhere in the eighteenth century was the range and quality of technical education greater than in post-revolutionary Paris. Furthermore for its time that education was remarkably democratic—strict entrance examinations ensured that (Aguillon, 1889 ; Birembaut, 1964). The first trained geological observers in Australia represented that French tradition. Unfortunately they were confined to coastal areas for the interest of their remarks stands in great contrast with those of the first inland explorers. The so-called mineralogists or geologists who accompanied some of the latter, for example John Oxley [1785?–1828], were generally little more than collectors.

Joseph Banks [1743–1820] had returned home with Cook in 1771 enriched with specimens and accounts of strange new plants and animals from the Pacific region. He returned to an instant scientific fame that his wealth and social place helped him to maintain for the rest of his life. By 1788 he had achieved considerable political influence. Banks's power as well as his genuine interest in the new country and its productions ensured that, despite the practical difficulties of existence in a remote penal settlement, colonial officials did their best to keep him informed and supplied with natural novelties.

Among various specimens sent by Phillip was one that attracted unusual interest although it had been despatched for a highly practical reason, namely the possible establishment in Sydney of a pottery. The Abbé J. A. Mongez [1751–1788], distinguished scientist and sometime editor of the *Journal de Physique*, while at Botany Bay with the La Pérouse expedition suggested that Phillip ought to have the local clay tested by a ceramist. Banks was asked to arrange this and passed a sample to his friend the potter-chemist Josiah Wedgwood [1730–1795] who, in addition to having a handsome medallion made (Rathbone, 1886), announced discovery of a new elementary substance to which he gave the name Sydneia (Wedgwood, 1790). Another part of the sample Banks gave to the Göttingen naturalist J. F. Blumenbach [1752–1840] ; his observations (Blumenbach, 1791) generally agreed with Wedgwood's. Blumenbach, incidentally, is credited by several writers (e.g. Lang, 1834) with a view that implies the ultimate in Australian novelty—that the continent arose by a cometary impact with the earth. I have failed to discover the original reference. But to return to Sydneia, the new element quickly found its way under that name or one of its synonyms Sydney Earth, Terra Australis or Terre Australe, Australa, Australsand and even Cambria (the latter, Forster, 1795) into treatises on systematic chemistry and mineralogy.

Its fame was short. In 1797 the Berlin chemist M. H. Klaproth [1743–1817] reported his failure to confirm the earlier observations and for his trouble was attacked by Nicholson (1797). The argument ceased with the publication of Charles Hatchett's [1765–1847] study of another moiety of Banks's stock (Hatchett, 1798). Impure reagents seem the most likely cause of the brief Sydneia affair ; Wedgwood had been thus deceived on other occasions (Schofield, 1963, p. 302). Blumenbach must have had like trouble though his results suggest a less contaminated acid. The nature of the impurity remains uncertain ; the responses observed would be compatible with the presence of bismuth in the hydrochloric acid.

Over the years Banks became a sort of scientific agent for the colony. The young Robert Jameson [1774–1854], later one of Werner's staunchest disciples in Britain, recalled seeing at Banks's house in August 1793 a sample of ' Sporadic Zeolite ' (i.e. the mineral in its host rock) just arrived from Van Diemens Land (Sweet, 1965). The Irish chemist Richard Chenevix [1774–1830] obtained from Banks the Australian mineral sand in which he discovered (Chenevix, 1801) the mineral menaccanite, a variety of ilmenite. Unfortunately the exact provenance of the sample is unknown ; the locality Providence Island mentioned by Chenevix arises from confusion with another new source in North America (cf. Leonhard, 1808, pp. 244–5).

Banks in fact went far beyond merely sitting in receipt of communications from government officers. Within ten years of the first settlement he had arranged, at his own expense, for a naturalist-collector George Caley [1770–1829] to work about Sydney (Currey, 1966). Caley's concerns were principally botanical but one finds scraps of geological information among his notes. The incomparably more scientific Robert Brown [1773–1858] owed his place as naturalist to Matthew Flinders's [1774–1814] expedition in Australian waters to the interest of Banks. In 1816 Allan Cunningham [1791–1839], another botanical protégé of Banks, arrived here to begin a notable career that included some of the first scientific exploration of the Australian inland. His journals contain much of interest to a geologist (Lee, 1925).

Among the affluent savants of Europe who did not share Banks's privileged position a rising passion for cabinets of natural history had to be satisfied by dealers. Few devotees of science were quite as detached as the celebrated French geologist Nicolas Desmarest [1725–1815]. According to Cuvier (1819, p. 370), Desmarest, when told of Cook's encounter with the Australian barrier reef, merely asked was the reef basalt or limestone. The demands of collectors led the more enterprising dealers to issue instructions to travellers about how and what to gather (e.g. Mawe, 1804). Within a few years topaz from ' Botany Bay ' was cheaper and more easily obtained in London than its Scottish rival (Thomson, 1814). The availability of such material led to its scientific study. Brewster's (1827) optical study of Australian topaz is but one example ; Brewster claims that the mineral was analysed by the great J. J. Berzelius.

When in 1803 the British government responded to requests from Sydney that a mineralogist be appointed to the civil establishment of the colony to assist a search for useful materials, the place went, on the recommendation of a wealthy collector, the Rt. Hon. C. F. Greville, to Adolarius W. H. Humphrey [1782?–1829], one of a family of dealers in natural history (Whitehead, 1973). Humphrey's contract permitted him to collect on his own account and ship samples free of charge. None of this material has yet been identified. We really know little of his work in the period 1803–12 that he enjoyed the office of His Majesty's Mineralogist but, if we may judge from the repeated requests for appointment of a successor, Humphrey cannot have destroyed faith in the value of his profession. No action, in fact, was taken until John Busby [1765–1857] in 1823 secured the post of civil engineer and mineral surveyor with particular responsibility for the coal mines and water supply. It is worth noting that a geological survey was not established in the United Kingdom until 1835.

National and provincial institutions appear to have been as eager as private collectors to acquire foreign material. The greater museums even followed the example of dealers. No fewer than five editions of *Instructions pour les Voyageurs* were issued by the Museum d'Histoire Naturelle, Paris, between 1825 and 1860. The collections made by the French expeditions passed mainly to the Paris museums ; the British Museum, the Geological Society and various local museums in Britain gained by way of donations and purchases from colonial officers and travellers. Acquisitions listed in the annual reports of the Geological Society provide a useful key to the collecting side of Australian geology.

Interest in museums and cabinets was no less strong in those states that had not yet manifested their naval pretensions by sending expeditions to distant parts. The German Travelling Union for Promoting Natural History (Hooker, 1827, 1830) which despatched collectors on behalf of subscribers offered one sort of solution. We know that P. E. de Strzelecki [1797–1873], J. Lhotsky [1795–*c.* 1870] and J. Menge [1788–1852] who were active in Australia during the 1830's and 1840's all had connections with European collectors. Income from the sale of specimens enabled them to pursue their valuable work as private scientific explorers.

Through the study of Australian collections in Britain and Europe many notable mineralogists and palaeontologists, some whose names appear on the maps of this country, came to a vicarious involvement in the growth of Australian science. Indeed their contributions were essential in the early years. The first skilled palaeontologist to settle here arrived at the end of 1854 !

By 1800 the colony was sufficiently established to attract free settlers who, with a few retired officials, set up as landed proprietors assisted by a plentiful supply of assigned convict labour. Jukes (1867) comments sharply on the ' landed manner ' of the first councillors of the Geological Society of London (founded 1807). These amateurs of science were men of property and social standing like Banks himself. The record of early nineteenth century geology in England shows that some were genuinely concerned with scientific enquiry and did notable work. One might expect that such gentlemen of science and their more affluent professional colleagues, were they to remove to the colonies, would find much to catch their interest. The first of them in Australia was a sad disappointment.

Dr. Robert Townson [1763–1827] came to Sydney as a settler in 1807. He arrived not only with a letter of recommendation from Banks to the governor (William Bligh [1754–1817]) but with a library, a laboratory (at least he had a grant to establish one) and the promise of free land. Townson had studied at Edinburgh and Göttingen and, moreover, was author of three books as well as several scientific papers. His *Travels in Hungary* (1797) includes a coloured map showing the distribution of different sorts of rocks in that country. He called the information *petrography* ; I know of no earlier use of the term. The *Philosophy of Mineralogy* (1799) and *Tracts and Observations in Natural History* (1799) demonstrate both his close awareness of the state of mineral science and his skill as an observer.

Despite this promise, letters written in 1800 (Pinkerton, 1830, Vol. II) suggest that Townson had already retired from science to the country. Why he decided a few years later to re-assert the scientist-image and move to Australia remains a mystery though he did have a military brother in the colony. In New South Wales it is plain he was far less enthusiastic for science than for farming and for the factional disputations that then consumed so much of colonists' time and energies. The select list of Townson's library (*Sydney Gazette*, Nov. 2, 1827) offers a mute commentary on a scholarly resource that might have been of great value in a remote community. But it may not be fair to blame Townson only for this failure. After all, Bligh writing to Banks in November 1807 and remarking on these very books said " many can be of no general benefit here, as they are written in the German tongue ". Such a society was no congenial place for scholarship.

The fate of the Philosophical Society of Australasia, founded in 1821 and expiring little more than twelve months later in " the baneful atmosphere of distracted politics " (Field, 1825, p. v), shows how small was the improvement since the time of Townson's arrival. Yet during its brief existence the Society had listened to original papers, of which a selection is preserved in Field (1825), and set about arranging a museum and exchanges with institutions abroad (Branagan, 1972*b*), though with what success is unknown. At least the idea of a museum did not die for in the year (1827) that James King [1800–1857] arrived in Sydney and reported so scathingly on the lack of science here (King, 1828) provision was made for the establishment of a Colonial Museum. This became, by 1834, the Australian Museum, the oldest existing institution in this country devoted to the natural sciences. To our shame, a history of the Museum compiled by Mr. G. P. Whitley remains unpublished ; it is a record that deserves to be known. But we must not exaggerage the Museum's contribution to geology in its early years when the greatest problem was survival. In fact the main advances in New South Wales in the two decades from 1830 came from the slowly increasing number of learned amateurs, at last drawn to a colony beginning to shed its convict image.

During that period, as Hoare (1968) has shown, the focus of organization of Australian science shifted from the old colony to Van Diemens Land (Tasmania). Even there the active students of geology were few, the most notable perhaps being the surveyor-general George Frankland [1800–1838] and Joseph Milligan [1807–1884]. Many of the select band who supported the Van Diemens Land Scientific Society and its successors may have been as much attracted by considerations of society as by considerations of science. Despite its own share of distracted politics that impinged on science, Tasmania has the proud record of the longest tradition of scientific serial publication in Australia. Since the first issue of the *Tasmanian Journal of Natural Science* appeared in August 1841 (Plomley, 1969), the societies of the island colony/state have continued to provide means of disseminating knowledge. Comparable journals did not emerge on the mainland before the 1850's. Until then resident scientific observers had to make do with local newspapers, almanacs and the few ephemeral literary magazines—or publish overseas. These local sources of information merit close study but are still quite inadequately explored.

Unlike the other colonies in which scientific societies emerged slowly and survived with difficulty, South Australia had a Literary and Scientific Association two years before the colony was proclaimed in 1836. This London group appears for a time to have been active ; among the addresses listed for 1834 was one by W. M. Higgins on the geology of Australia (Hale, 1956). An insufficient number of these philosophical gentlemen must have moved to the new colony for the Association seems not to have continued in Adelaide. But from the first settlement there was a particular geological interest in South Australia. The South Australian Company briefly (1836–8) employed a geologist, Johann Menge, a strange character if we believe Cawthorne (1849). Menge's (1841) treatment of South Australian geology is certainly original. Papers on local geology by Binney (1842) and Finniss (1843) belong to the first few years of the colony as does the first book on geology published in this country (Burr, 1846). The discoveries of lead and copper deposits in 1841 and the years following (Price, 1924, p. 231) rewarded such interest. On the other hand the early success of mining in a region where few fossils had been found probably helped maintain in South Australia the themes of prospecting and mineralogical geology at a time when the search for stratigraphic order was beginning to yield results in the coalfields of eastern Australia. That search and indeed practically all activity—political, commercial and scientific—in Australia underwent profound change with the advent of gold-fever in 1851. To the years following belong establishment of the first universities and the beginnings of geological survey. Both subjects have been discussed by Andrews (1942) and, in the palaeontological context, by Brown (1946).

European Prejudice and Geology in Australia

I turn now to consider how a few of the concepts and methods of geology, derived from European experience, fared in translation to a part of the world not only remote but largely unknown. Even in the more sophisticated society of eastern North America there seems to have been a certain reticence to theorize. Ospovat (1967) points to an apparent reluctance among the pioneers of American geology to take sides in the neptunist-volcanist dispute but emphasises the profound influence on them of Werner and his school in the period from 1785 to *c.* 1830. A recent commentator on early Australian geology (Seddon, 1973) makes a case for Wernerian influences in this country and goes so far as to claim that some of our early naturalists preferred the ' cold-bath ' of neptunism. I believe, however, that we must exercise care in seeking to identify influences of particular schools of thought on our pioneers. After all, their knowledge of science was most likely to have arisen through accidents of experience related to travel and conversation more than to special reading and conscious study.

When we encounter an observer inferring volcanic action, it is unwise to pronounce the person a follower of Hutton. Ideas of heat and fusion in the earth existed long before Hutton's time. Similarly, one who used terms favoured by the Freiberg professor need not have been an advocate of neptunian precipitation. Long after support for that model had waned in Europe many geologists continued to employ the language of Werner's stratigraphic and mineralogical systems—some of his mineral names still continue to be used though one would search long and hard for a modern believer in the ' cold-bath '.

It is convenient, however, to begin a search for European influences among the crucial areas of the neptunist-volcanist debate, a debate that involved interpretations of earth-processes, how rock masses originated, how they came to be disposed as now seen and how the earth's present configuration arose. I shall start with the obvious question of igneous phenomena and go on to consider a few other aspects of what we now term physical geology before ending with a look at the way a search for order in time was developed in Australia. Throughout, I believe, we shall see European prejudices dominating Australian experience.

The very idea that Australia was somehow a land of anomalies, an idea still not lost from popular thought and for long expressed in serious geological literature (e.g. Rattray, 1869), derives from such prejudice. The early explorers found a continent the physical features of which differed utterly from those of Europe; instead of a great median mountain axis in Australia there were low arid plains, the mountains of Australia followed the east coast. Such physical anomalies presumably matched the equally ' anomalous ' flora and fauna. Despite recognition of such differences, the geologists of Australia long continued to fit their science into the European mould.

Igneous Action

Here was a central issue of dispute between the schemes of Hutton and Werner. Hutton with his belief in heat as a geological agent found no difficulty in accepting the possibility of fusion of rock matter and the possibility that such action could take place from time to time in the earth's history. The Wernerian neptunist by force of observation accepted the existence of volcanoes but assigned to them a recent and superficial role and regarded them as due to local fusion occasioned by the ignition of combustible matter such as coal or sulphur. The term *basalt*, as used by Werner, implied no synonymy with *lava*, only the latter was associated with volcanic phenomena ; crystalline rocks in general (basalt, granite, etc.) resulted from aqueous precipitation. Let us now look at some examples of the way the subject was treated in Australia ; they suggest that strict neptunian opinion arrived late and had little influence.

Matthew Flinders, in his journal of a voyage to the Furneaux group in Bass Strait during 1798, offers some remarkable opinions that merit quotation. After describing granite insinuated among strata of slate on Cape Barren Island, he says :

> " Granite being the heaviest of all stones, must, according to geologists, have been placed nearer than any other to the center of the earth, in its primitive state ; and being situate so near the fountain of heat and fire, might be dissolved, when the more light bodies, such as slate, were not, or perhaps could not. When the universal burst happened, which some suppose to have taken place in consequence of the sudden rarefaction of the air and water, which had gained admission to this internal region of fire, these bodies might naturally become thus compounded ; the liquid substance filling up every interstice, which its fluidity and superior gravity enabled it to penetrate " (Flinders, 1798 (1974, p. 11)).

Despite the insinuation of granite melt there is nothing of Hutton and uniformitarianism here. In the passage Flinders seems to hark back to Buffonian catastrophism or perhaps to John Michell [1724–1793] and his theory of earthquakes. Where would a naval officer who had gone to sea as a boy acquire such ideas ? Is there a hint to be found in his old chief William Bligh ? Bligh's remarks on the vicinity of Adventure Bay, Tasmania, written in 1792 when Flinders was there as a midshipman, indicate his acceptance of heat as a geological agent (Lee, 1920, p. 26). At any rate Flinders's later writings (Flinders, 1814) show his continuing faith in igneous action, a faith that makes his membership of the Wernerian Society of Edinburgh somewhat unexpected (Sweet, 1967, p. 211).

The French expedition of 1800–04 sent out under the command of Nicolas Baudin [1754–1803] is of particular interest because of the presence of two trained mineralogists, Louis Depuch [d. 1803] and J. C. Bailly [1777–1844], in the scientific party. Baudin (1974, p. 232) quotes a report of the naval engineer Ronsard who led a party ashore on what was to be called Depuch Island, off the coast of Western Australia. Ronsard identified basalt, which he also terms lava, on the island. Depuch's description of a sample is given by Péron (1807, p. 131).

The two mineralogists were both recent graduates, Depuch of the Paris École des Mines where he had studied under Haüy, and Bailly of the École Polytechnique. It is reasonable to expect them to express informed and up-to-date opinions. That Depuch Island was not the scene of some aberration is clear from their later remarks on the volcanic nature of the Tasmanian basalts (dolerites). Although readers of the official account of the voyage (Péron, 1807 ; Péron and Freycinet, 1816) might be forgiven for thinking that Bailly, like his colleague Depuch, had succumbed before the record was written, he survived to follow a most distinguished career as a naval hydrographer. Furthermore, Bailly's account of extinct volcanoes on the island of Mauritius contributed to a book by his friend J. G. Milbert and published at Paris in 1812 leaves no doubt of his igneous beliefs. Bailly (1825) is derived from a German translation of Milbert issued in that year.

These French reports of volcanic rocks in Australian localities where no volcanoes and no combustible materials had been observed created some stir in Europe. The most usual response by neptunists was to ascribe the rocks to Werner's Trap Formation. Perhaps it was with the French work in mind that Karsten (1808, p. 83) placed basalt of the Trap Formation at Botany Bay—to someone in Europe that place and New Holland may have seemed much the same. On the other hand, it is not hard to find other German writers of the period accepting volcanic action in Australia despite the seeming absence of volcanoes (e.g. Zimmermann, 1810, p. 848); several were clearly aware of Pallas's scheme that required volcanic action in the South Seas.

Although recent work has not confirmed the volcanic nature of Depuch Island—the dark rocks there (Ryan, 1966) like those seen by the French in Tasmania are now regarded as intrusive dolerites—we may recall that F. T. Gregory [1821–1888] in 1861 claimed to have found evidence of volcanic action on the adjacent mainland. Gregory (Gregory and Gregory, 1884, p. 71) believed that volcanic heat had there been " sufficient to convert the trap and sandstone into a deep bluish-grey scoria " even though no " actual overflow of lava " was observed. In this area southeast of Roebourne, dolerites like those of Depuch Island as well as Proterozoic lavas are now recognized.

A much earlier instance of faith in fusion is expressed by H. C. Antill [1779–1852] in an account of a journey across the Blue Mountains in 1815. Antill saw on King's Tableland what he took to be evidence of a violent volcanic eruption that had produced material like melted sand (Mackaness, 1965, pp. 88–89). The occurrence elicited a different response from Fedor Ivanovich Stein [d. *c.* 1845], surgeon and naturalist to the northern section of the Russian Pacific expedition of 1819–21, who visited it in the company of Allan Cunningham (Aurousseau, 1972). Stein (1842) made no secret of his respect for Werner, whose name together with those of two Russian mineralogists he attached to topographical features that cannot be identified. The phenomena observed by Antill and Stein are now ascribed to diagenetic cementation. In this case the model involving aqueous precipitation has fared better than that of fusion. The example is not unrelated to the problem of flints to which Hutton (1795) gave misdirected attention.

If Stein is one of the few avowed Wernerians among early observers of Australia, Alexander Berry [1781–1873] emerges as an equally rare professed Huttonian. His comment " Dr. Hutton would have given much for a single day's walk along this shore " south of Lake Macquarie (Berry *in* Field, 1825, p. 235) bespeaks an attachment probably formed while a medical student at Edinburgh. Throughout that paper, the first geological work prepared and presented (March 6, 1822) in Australia, Berry expresses Huttonian doctrine.

Stein for all his attachment to Werner was no strict neptunian.* He mentions volcanoes in the Pacific region conformable to the beliefs of that earlier Russian resident, Pallas, and has left an account of an active volcano in Kamchatka. Stein was certainly a catastrophist but in that he differs little from the majority of our geological pioneers.

Despite the various reports of volcanic rocks in Australia no one had found an active volcano. The Burning Mountain or Mount Wingen, found in 1828 in the Upper Hunter district of New South Wales, was therefore bound to attract attention though it came too late to comfort many but the most persistent neptunists. Long before, in 1801, James Grant [1772–1833] had examined the Hunter River as far as Singleton and remarked, for reasons undisclosed, that the

* Dr. John Henderson (Hoare, 1968) seems to be a more worthy candidate. Henderson (1832) not only employs Werner's stratigraphic terminology (many non-neptunists did that) but his discussion of crystalline rocks of the Wacke Stratum in Australia is distinctly neptunian.

minerals " about the river in general are volcanick " (*Hist. Rec. N.S.W.*, IV, p. 408). It is highly doubtful if the Rev. C. P. N. Wilton [1795–1859] was aware of this when he visited the Burning Mountain in 1829 and, indeed, the two matters are unrelated. Wilton wrote an account of his visit for the *Sydney Gazette* (March 14, 1829) ; see also Wilton (1830). " That celebrated mountain of the southern world " (Wilton, 1832, p. 186) was mapped by T. L. Mitchell (1838, Plate 9) in the very month of Wilton's first visit though neither in their published remarks acknowledges the other. The only authorities worth mentioning apparently lived in Europe. At least Mitchell and Wilton both agreed on the cause of the phenomenon at Mount Wingen, combustion of a coal seam, though Wilton's slight attempt to consider the occurrence in the context of known volcanic regions may betray a vestigial Wernerian attitude.

Reference to this pseudo-volcano of Australia will be found in a few geological treatises of the period. Pseudo-volcanic action of a different sort was invoked by Menge (1841, No. 7) to account for opaline silica and siliceous tufa in South Australia. As a young mineral collector, Menge had been to Iceland in 1819 where he was impressed by the hot springs ; the siliceous deposits of his new home he ascribed to a like cause. On Iceland, Menge (1820) was essentially Wernerian though apparently not pure enough for his editor Jameson, who added his own remarks to the paper. In Australia the hermit Menge praises Werner for his mineralogy but develops a line all his own.

Mitchell's explorations of 1836 in western Victoria revealed the existence of well-preserved craters and cones (Mitchell, 1838). Examples, such as Mount Napier, are described without any particular effort to justify their diagnosis as extinct volcanic centres. To Mitchell, who knew Lyell's *Principles*, trap and amygdaloid were simply igneous rocks. The distinctive features known to the settlers of that region as *Stony Rises* are considered by Westgarth (1846) as products of extinct volcanism. In the southeastern corner of South Australia Mounts Gambier and Schank were recognized as extinct eruptive centres by Burr (1844). Thomas Burr [fl. 1839–1860], for a time deputy surveyor-general of South Australia and then superintendent of the Burra Burra mines, is an interesting figure who deserves further study ; I know only of his career in South Australia. Another South Australian resident, B. T. Finniss [1807–1893], in his account of local geology (Finniss, 1843) considers evidence of volcanism far more ancient than that represented by the craters and cones seen by Mitchell and Burr.

From that time forward one finds few if any signs of hesitation in recognizing igneous rocks in Australia. In some forty years treatment of such rocks here had passed from an old-fashioned approach derived from the days before Hutton and Werner to one influenced by Lyell. That development had taken almost twice as long originally in Europe. The telescoping of the process in this country was achieved by a diminished attention to both strict neptunism and Huttonian volcanism.

Relative Sea-Level Changes

J. R. Forster [1729–1798], naturalist with his son Georg to Cook's second voyage, reminds us that even in his time diminution of sea level and emergence of land were subjects of interest (Forster, 1778, p. 145). Forster had spent some years in Russia and probably was aware of Pallas's work ; indeed he may have known the man. Forster's suggestion that the steep rocky southerly terminations to the southern continents resulted from a violent flood from the southwest seems to owe much to Pallas.

Our record of inferred coastal changes based on direct observation begins with a visit of George Vancouver [1757–1798] to King George's Sound in 1791. There he found calcareous material (he called it coralline limestone) capping hills well above the present level of the sea (Vancouver, 1798, I, p. 49). No doubt the ' coral ' was fossilized vegetable matter but our interest lies in Vancouver's conclusion that the land had only recently emerged. Some ten years later both Baudin's and Flinders's expeditions examined that and many other localities along the western and southern coasts of the continent and noted the considerable extent of the calcareous rock known still as the coastal limestone.

The French, in particular, were impressed to find in the limestone remains of organisms like those inhabiting adjacent waters. Some, for instance *Trigonia*, were known in Europe only as fossils. The discoveries reinforced others made earlier by R. P. de Lamanon [1752–1787], one of La Pérouse's naturalists. Lamanon's descriptions of living terebratulids and nautiloids from the eastern seas survive to remind us of the scientific potential in that ill-fated expedition (Milet Mureau, 1798, II, pp. 321–339). François Péron [1775–1810], one of Baudin's zoologists, quickly seized on the point that the Australian region was a habitat for ' living fossils '. That theme has been repeated many times since and there is no denying the relevance of observations of Australian habitats and life-styles of such as the Port Jackson Shark, the marsupials, various molluscs, brachiopods and corals, not to mention elements of the extant Australian flora, in the development of European palaeoecology. The title of Unger's little treatise on aspects of the Tertiary flora of Europe—*Neu-Holland in Europa* (Unger, 1861)—indicates something of this return influence.

Soon after Péron's return to France he read a paper on zoological facts applicable to geology (Péron, 1804). It stands as ancestor to Chapter XXVIII of Péron and Freycinet (1816), but with some interesting differences. For one thing, the tortuous argument later offered to justify ignoring place names given by Flinders is all the stranger when one finds that Péron in 1804 was content to follow the English navigator. Again, Bailly the mineralogist, acknowledged as a source in the earlier issue, is dropped from the revision where only the dead Depuch is praised. Over the years Péron has been covered with glory ; Tate (1894) adds his share. For an alternative view I suggest a reading of Baudin's (1974) journal and the translator's preface to Dégérando (1969).

But no matter how we regard the man, Péron (1804) provides many useful details on the distribution of organisms, especially of corals, in warm seas. He argues that the coastal limestone of Australia must be of recent origin because its fauna so closely resembles that still extant nearby. Indeed both the French and Flinders* believed the material was still being formed at sea-level. Those occurrences now above the sea Péron saw as evidence that the land had emerged from the sea, an emergence he attributed to sudden and violent revolution. His catastrophism harks back to Buffon and Pallas and brought him wide notice, the more so after Cuvier's espousal of the doctrine. Cuvier cited Péron and New Holland in support of his argument (e.g. Cuvier, 1813, 1825) but gave no prominence to the ' living fossils '. His revolutions wiped out populations so living organisms ought not to be too closely related to fossils.

Péron's catastrophist model was taken up by J. B. Lamarck [1744–1829] whose great work *Histoire Naturelle des Animaux sans Vertèbres* (1815–22) was to include details of both living *Trigonia* gathered by Baudin's naturalists and fossil forms (cf. Deshayes, 1831) hardly supportive of Cuvier. Lamarck (1805) follows a geological theme outlined in a highly original but neglected treatise (Lamarck, 1802). Lamarck saw no need to postulate violent action. Indeed

* Flinders (1814, II, pp. 115–6, 336) also made valuable observations on the coral reefs off the northeastern coast. His scheme of gradual progressive growth of the reefs is not unlike that suggested by Forster (1778, p. 150) for the origin of the so-called low islands of the Pacific. Both works were recognized as important by later students, notably Jukes (1847, 1850), of the subject in Australia.

his broadly uniformitarian view would hardly have affronted Hutton though there is no evidence that he even knew of the man or his theory. Lamarck commented on various ways of achieving gradual decline of sea level (Péron had not concerned himself with the cause of his revolution). Consumption of water by organisms or through volcanic action Lamarck rejected as insufficient and concluded that sea level varied from place to place through time consequent on migration of the earth's equatorial protuberance—a sort of embryonic model of polar wandering—and adduced astronomical detail in support. E. M. L. Patrin [1742–1815] joined the discussion (Patrin, 1805) as a protagonist of volcanic consumption while sharing a preference for gradual adjustments as opposed to catastrophic revolutions.

These alternative schemes passed practically unnoticed. Catastrophism was a received doctrine in Europe ; its persistence in Australia, as we shall see, was longer and at least equally strong. Nevertheless, after this active beginning the investigation of relative sea-level changes in Australia faded in popularity although valuable observations were made during the marine surveys (King, 1826) of P. P. King [1791–1856], during the visit of J. R. C. Quoy [1790–1869] and his companions to Shark Bay with the French expedition of 1817–20 (Freycinet, 1828, pp. 471–476) and in the course of the voyage of H.M.S. *Fly* (Jukes, 1847). Remarks of a general nature are to be found in Mitchell (1838, II, pp. 368–9) and in Dana (1849, pp. 533–6). Sommer (1848, p. 45) comments on the evidence of apparently rapid changes in coastal configuration in the Hutt River estuary of Western Australia and Gregory (1861, p. 482) mentions the natural uncovering of a ship buried further south on the same coast. I can find no earlier map of old shorelines than that of the lower Macleay River area, N.S.W., compiled by E. W. Rudder (1867).

The Formation of Valleys

Both Hutton and Lamarck had argued for fluvial agencies as sufficient causes in the formation of valleys but early nineteenth century opinion still favoured concepts popularized by Pallas, that valleys were carved by the action of catastrophic floods. Wernerians found nothing there with which to take exception and neither did the Cuvierian catastrophists.

From the early days travellers in New South Wales have been impressed by the valleys of the Blue Mountains. These vast features bounded by near-vertical cliffs seem to be entirely out of scale with the small streams that inhabit them. Gregory Blaxland [1778–1853], one of the first party to succeed in crossing the mountains in 1813, suggested that the valleys were manifestations of " an earthquake or some dreadful convulsion of Nature " (Mackaness, 1965, p. 7). R. P. Lesson [1794–1849], naturalist to Duperrey's expedition of 1822–5, reflects a like catastrophist view in his notes of a visit in 1824 : " It must have required a very great movement to rend so perceptibly this section of the Blue Mountains " (Mackaness, 1965, p. 152). Earthquakes rather than floods were the more popular cause. A few tremors had been felt about Sydney since the first was reported in June, 1788 ; Mann (1811), with remarkable hyperbole, even claimed the colony was menaced with destruction by an earthquake on January 17, 1801. Perhaps knowledge of these aided adoption of the particular catastrophist model.

By the time of the visit of Charles Darwin [1809–1882] in 1836, opinions in Europe were changing. Lyell and Murchison (1829) had revived the case for fluvial agencies in their account of valleys in the Auvergne. Aware of his regard for Lyell, we might expect Darwin to follow this line but his conclusion is very different. Darwin found the valleys among the few aspects of Australia with which he could be impressed but decided that " to attribute these hollows to alluvial action, would be preposterous " (Darwin, 1844, p. 136). In his notebook (Barlow, 1945, p. 251) Darwin merely asks the question " Cause of great precipice? ". In 1844 he comments that cliffs limiting the valleys reminded him of sea-cliffs, an observation conformable to the model of marine denudation he was to offer.

Marine currents as agents of erosion had long held appeal. Surely, we may ask, Darwin was too familiar with Lyell to enter such a catastrophist trap. Chorley *et al.* (1964) point to a likely explanation. Lyell himself had gone off at a non-uniformitarian tangent. The fifth edition of the *Principles* issued in the year of Darwin's return from the *Beagle* voyage (1837) presents a case for marine denudation in the origin of valleys. This Darwin appears to have swallowed whole. Darwin on Australian geology is no shining original.

In this department he is deservedly overshadowed by James D. Dana [1813–1895], geologist to the U.S. Exploring Expedition of 1838–42. Dana spent nearly four months of 1839–40 in and about Sydney making visits to the Illawarra district, of which he prepared a geological sketch map, and to the Hunter River area. He did not, in fact, cross the Blue Mountains but had the benefit of the experience of a colleague, Horatio Hale, who went to Wellington; Dana himself examined the Kangaroo Valley. He provides (Dana, 1849, p. 526ff) a thoroughly fluvial account of the history of the valleys and in so doing supports the earlier brief exposition of Mitchell (1838, II, pp. 351–2). In view of the inordinate rarity of Dana (1849)—see Haskell (1942, pp. 67–68)—it may be worth noting that the substance of his case is available elsewhere (Dana, 1850). With geologists of the calibre of Dana we see a reason for the remarkable development of North American geology in the second half of the nineteenth century. Would that he had found greater opportunity to explore Australia.

Reading Dana's argument one might expect that there the matter of the valleys and catastrophist notions also would rest. Only one later example must suffice to show the error of such expectation. W. S. Jevons [1832–1882], later famous as logician and economist, worked for some years at the Sydney Mint and while here interested himself in geology. In an unsigned article Jevons (1858, pp. 90–91) dilates on a thought that the valleys represent regions of collapse in ranges raised by volcanic action. That reminds one of Strzelecki (1845, p. 150) and his volcanoes of ' elevation '; and those he borrowed from European geology of the 1830's.

Diluvial Geology and Glaciation

It was mentioned earlier that especially in England the Flood of Noah came to have a geological dimension. Buckland's first major work (Buckland, 1823) deals with the remains of fossil vertebrates preserved in caves known as osseous caverns. These animals, argued Buckland, were victims of the great inundation.

The Flood caught up with Australia very quickly. In a popular account of the roads of New South Wales, Raymond (1832, p. 101) notes the natural feature called 'ploughed ground' in the colony as possibly " impressed on the earth when it first emerged from the Deluge ". C. P. N. Wilton (1828), in fact, introduced the subject of Diluvial Geology in a journal he started soon after his arrival in 1827. Wilton had taken a pass degree at Cambridge the year before Sedgwick became professor, though later correspondence indicates their acquaintance. Wilton's connection with the Oxford Ashmolean Society suggests also the attraction of Buckland. The Flood must have interested Wilton before he came to Australia for there are references (Boué, 1832, p. 145; Melleville, 1842, p. 5) attributing to him a commentary dated 1826 on the fundamentalist work of a British M.P., Granville Penn. Penn (1822) had claimed that what he termed Mineral Geology and Scriptural or Mosaic Geology were antipathetic and irreconcilable. Despite much search I have failed to trace Wilton on Penn; I imagine he essayed a moderate Bucklandian position. Lang (1846) and Rudder

(1854) provide later Australian accounts of Mosaic or Bible Geology before it ceased to have any pretension to science and passed to the pulpiteers.

Wilton (1828, p. 193) remarked presciently that bone deposits like those described by Buckland might be found in this country. " In or about 1830 " (Anderson, 1933, p. x) bone deposits in the Wellington Caves were brought to notice. Anderson gives an account of the discovery and subsequent study of these remains, a study that involved some of the leading comparative anatomists of Europe.

Apart from the purely palaeontological interest, the discovery of these osseous caverns had other implications, not least those relating to the age of Australia. Referring to the cave deposits, Henry Dumaresq [1792–1838] said in a letter from Sydney " it has been ascertained that the hypothesis, with regard to its [Australia's] post-diluvian formation, is as groundless as that of its absorbent interior marshes " (Barrow, 1832, p. 2). The shaken hypothesis was that based on the coastal limestone, that Australia had recently risen from the sea. As that emergence had been represented as violent there would appear to have been room for conflict between post-diluvial catastrophes and belief that the Flood (which had wiped out terrestrial animals) was the last such disaster. However, no one seems to have been greatly concerned. The German geographer Carl Meinicke [1803–1876], in an interesting review of Australian geology (Meinicke, 1837, I, ch. 4), simply concluded that the cave deposits indicated Australia was of greater antiquity than had been thought hitherto. Less than forty years later we find F. von Hochstetter [1829–1884], geologist to the Austrian *Novara* expedition of 1857–9, stating that Australia was one of the world's oldest continents and that most of the region had been subject to little disturbance since the end of Palaeozoic times (Hochstetter, 1864, p. XLVI). Behind that shift lie other revolutions—of thought and experience.

Diluvial theory in its heyday offered an explanation not only for the cave deposits but for the great spread of boulder clay across northern Europe. Hutton's first fluent disciple John Playfair [1748–1819] had pointed to the effectiveness of glaciers in transporting blocks of stone (Playfair, 1802, p. 388). By the 1830's a few Swiss geologists revived thoughts of glacial action ; the best-known though not the first of them, J. R. L. Agassiz [1807–1873], won some converts in Britain following his visit in 1840. Buckland, once the prince of Diluvialists, had already abandoned most of his catastrophist views (cf. Buckland, 1836) through Lyell's influence, so for him the step to acceptance of glacial action was not great. Geologists being a conservative lot continued to use the term Diluvium practically as a synonym for Pleistocene long after belief in the Flood as a pervasive geological agent had disappeared.

Thoughts of glaciers spread to Australia with remarkable swiftness. Murray (1842, p. 203) claimed to have seen evidence of moraines and other glacial features in the Pyrenees, WNW of Melbourne. The example has greater interest for its date than for its conviction. The Rev. W. B. Clarke [1798–1878] appears to have been the first to express belief that the Snowy Mountains bore evidence of glaciation (Clarke, 1860, pp. 225, 230). In that year A. R. C. Selwyn reported signs of glacial action in the striated pavements of the Inman Valley of South Australia (Selwyn, 1860, p. 4). Before coming to Australia Selwyn had assisted A. C. Ramsay [1814–1891], one of the first British students of terrains affected by the Quaternary glaciation. Selwyn seems not to have suspected that the glacial features of the Inman Valley belonged to a far older event but then who at the time thought the (Quaternary) Great Ice Age was other than unique ? Without realizing it Selwyn had come across one of those geological features that unite the southern continents and India and which caused so many difficulties to those with European prejudices. How close Selwyn came to breaking from this strait-jacket may be seen in his notion that glacial agencies had operated during deposition of what he took to be late Palaeozoic rocks at Bacchus Marsh, Victoria (Selwyn, 1861; Selwyn and Ulrich, 1866). One result of Selwyn's enforced removal to Canada and the triumph for a time of office-geology in Melbourne was a ' revision ' of the stratigraphy at Bacchus Marsh. Indirectly it saved for the geologists of India the credit for establishing the non-European character of late Palaeozoic successions in the southern lands. But here I trespass upon my final topic.

The Search for Stratigraphic Order

Whereas Werner's process of neptunian precipitation may not have been invoked by many of our pioneers there is no denying the influence of the method of lithological stratigraphy that was part of the Wernerian system. The search for order of succession in this country began, from the accidents of landfalls and settlement, in a distinctly haphazard way. But according to the concept of world-wide formations that Werner had made his own that need not have caused difficulties. The significant point was to recognize the characteristic lithologies.

Leopold von Buch took the trouble while in Paris during 1810 to examine the collections made by Bailly and Depuch; he found samples that generally matched rocks known in Europe. But what, Buch (1814) asked, was to be made of materials like the coastal limestone that bore fossils so like organisms still living? The rock could be assigned a place in the European order but there no such association with related living organisms was known. Were there, in fact, formations of local extent? The Paris collections supplied no answers. Buch's rhetoric is left undeveloped but it strikes at the core of the doctrine of universal formations by hinting that a particular sort of rock may not everywhere occupy the same stratigraphic position. Although once a student of Werner, Buch was no typical disciple; he had already abandoned belief in the neptunian origin of basalt and his interest in fossils was for the time quite unusual.

The Problem of Australian Coal

Buch knew that coal with sandstone, as in Europe, had been found near South Cape, Tasmania, during the second visit (1793) of the d'Entrecasteaux expedition (Labillardière, 1799, II, pp. 21–22) and suggested the fossil plants collected near North-West Bay by Baudin's naturalists might also belong to the coal measures. Here his European experience prevailed, the term used, *Steinkohlengebirge*, was that applied to the coal measures that in Europe lay just above Werner's Transition series.

Stratigraphic order in Australia really begins with coal, as it did in Europe. When Ludwig Leichhardt [1813–1848?] observed (Leichhardt, 1849, p. 45) that the geology of Australia in general started at Newcastle, N.S.W., we know what he implied. Coal had been found by runaway convicts on the coast north of Sydney in 1791. London papers reported the find (e.g. *The European Magazine*, July 1792, p. 77) but the news does not appear to have reached Sydney before the next discoveries were made in 1796. George Bass [1771–1803], sent by the governor to examine the occurrence at Coalcliff, south of Sydney, found the coal there to lie below the sandstones that abounded near the settlement. In a letter written from Sydney in August 1797 to a colleague then in London, Bass evinces the first signs of belief in a coal basin in the region (*Hist. Rec. N.S.W.*, III, p. 289).

Depuch and Bailly (Péron and Freycinet, 1816) saw in 1803 that shales lay above the sandstone near Parramatta and predicted that older (primitive) rocks must be exposed in the area drained by the Hawkesbury system. A few years before, in 1798, explorers to the southwest of Sydney returned with claims that salt, coal and limestone existed there (*Hist. Rec. N.S.W.*, III, pp. 820–8;

Cambage, 1920). The salt was said to form cliffs and veins (Collins, 1802, pp. 88, 98) ; from such reports arose stories of mountains of salt in Australia. In fact, the salt merely occurred as incrustations. Not long after, Humphrey the mineralogist found salt in the midlands of Tasmania, another area where coal was known.

To the lithological stratigrapher these finds appeared to offer scope for correlation. In Europe salt deposits existed in what came to be called the New Red Sandstone. Below this, in order, came first Magnesian Limestone then, as a rule separated from it by an unconformity, came the coal, shales and sandstones of the Coal Measures, the quartzose sandstones of the Millstone Grit and finally limestones known as the Mountain Limestone. The latter in England rests on Old Red Sandstone. A valuable contemporary treatment of this sequence is given by Conybeare and Phillips (1822) ; in more modern terms the succession ranges from Triassic to Devonian. The Old Red with the slates and greywackes below were grouped in Werner's Transition series.

These various terms appear in the early literature of Australian geology. Predictably, red sandstones became Old or New depending on their relation to the coal or, where that was unknown, on individual choice. Mitchell and Sturt, for instance, seem to have preferred the Old as a matter of course. Those who relied on collections made by others were no less arbitrary. The report of Humphrey's examination in Sydney of samples gathered by F. L. Barrallier [1773–1853] on his journeys of 1802 into the Blue Mountains allows us to glimpse the limitations of local science. Humphrey decided that Barrallier had not really reached far into the mountains because no granite had been brought back (*Hist. Rec. Austr.*, ser. I, vol. V, p. 589). The argument might have interested Pallas or Werner but Humphrey had evidently failed to examine the explorer's journal (*Hist. Rec. N.S.W.*, V, Appendix A). In that document the explorer not only reports the occurrence of granite but also notices the presence of fossil shells in the Kowmung River area. These fossils are now recognized as the first Devonian remains found in Australia. Perhaps Humphrey had no French and so was unable to study the journal.

N. J. Winch [1768–1838] of Newcastle-upon-Tyne decided from a study of collections made by the Rev. T. H. Scott [1783–1860] that apart from diluvium there was nothing in Tasmania younger than the European Magnesian Limestone and in New South Wales nothing more recent than the Coal (Winch, 1823). Scott (1824) himself offered some notes on the supposed distribution of the various units identified on the basis of lithological features. The stratigraphy recognised by W. H. Fitton [1780–1861] from a study of P. P. King's collection and from published sources depends also on what had become almost standard lithological criteria (Fitton, 1826). By that time, however, workers in Europe had started to recognize the value of fossils in correlation.

Buckland (1821) from an examination of Robert Brown's collections made in 1803–5 decided plant fossils in the Australian coal were like those of England and that marine fossils from near Hobart resembled those of the Mountain Limestone. Yet it was among samples in Buckland's possession that Adolphe Brongniart [1801–1876] recognized *Glossopteris browniana* (Brongniart, 1828), a plant he found also in Indian coal but quite unknown in the European coal measures. This distinctive southern coal plant appears earlier to have been confused with *Eucalyptus* (cf. Scott, 1824). R. P. Lesson (Lesson and Garnot, 1826, p. 6), in fact, went so far as to postulate a Tertiary age for the sandstones of the Blue Mountains, presumably because of the apparent relation of the fossil plants to living types—an interesting example of the then newly-emerging palaeontological method foundering on erroneous identification.

Quoy in 1826 collected casts of spiriferids from near Jervis Bay, N.S.W., that were deemed to resemble those of the " période Phylladienne ou intermédiaire " in Europe (Dumont d'Urville, 1830, pp. cix–cx), i.e. Devonian. Fossil shells from below the coal in the Hunter Valley are noted by Wilton (1832) and Mitchell (1838). Mitchell, in particular, took the enterprizing step of enlisting help from the palaeontologist J. de C. Sowerby [1787–1871] (cf. Cleevely, 1974). The spirifers suggested a correlation with Mountain Limestone. G. B. Sowerby [1788–1854] assigned a like place to the brachiopods gathered by Darwin during his visit to Tasmania (Darwin, 1844). Darwin's corals from the same area were thought by William Lonsdale [1794–1871] to be of Devonian or Carboniferous age. Another interesting collection of rocks and fossils from New South Wales and Tasmania is listed by Chevalier (1844). P. E. de Verneuil [1805–1873] referred the Tasmanian fossils to Carboniferous types (Verneuil, 1840). Although a few years earlier H. T. De la Beche [1796–1855] had warned there was no real evidence that the coal of Australia must be of an age with that in England (De la Beche, 1835, p. 306), the palaeontologists by suggesting correlation of marine beds below the Australian coal with the Carboniferous Mountain Limestone can only have strengthened belief in a European-type succession at the antipodes. Any distinctive features associated with the Australian coal perhaps came within the province of ' wayward sports ' that Nature here turned up to ' amuse ' the geologist just as she did in the animal kingdom (Wilton, 1833, p. xviii).

As yet no one had attempted to examine relations of strata in the field or to map the distribution of units including the coal. Credit for the first such work belongs to P. E. de Strzelecki [1797–1873]. His map, of which a reduced version accompanies his important book on Australia (Strzelecki, 1845), has been examined recently by Branagan (1974). The man himself still presents many problems. We know for instance almost nothing of where he learned his geology (Heney, 1961, pp. 41–2) yet clearly he had a good working knowledge of the subject. The particular systems of geognostic divisions he adopted for Australia (Strzelecki, 1841, 1845) reflect continental influences but these are treated with unexpected independence. His approach incidentally is followed almost in its entirety by Grange (1854). There is catastrophism in Strzelecki's grouping of epochs each separated by a revolution but it is not Cuvierian catastrophism. I see it rather as reflecting the evolved model by which Elie de Beaumont [1798–1874] and others were leading geology towards concepts of orogenesis.

Strzelecki's (1845) attributions of rocks in particular localities to particular epochs do not in every case bear close examination but the wonder is that he covered so much ground during the four years of his stay here and furthermore managed on his own resources. His treatment of geology in the book is largely lithological and distributional ; apart from the map the main interest now lies in the work of John Morris [1810–1886] on Strzelecki's fossil plants and of Lonsdale and Morris on the fossil fauna. Morris's remarks (Strzelecki, 1845, pp. 252–3) on the apparent differences between the coal floras of the northern and southern hemispheres and the contrast of these differences with similarities exhibited among fossils from the older rocks of both hemispheres may seem to us obvious : in his time they represented a necessary recognition. Some more rational answer than wayward sports of nature had to be found.

For all the interest of his mapping Strzelecki did not succeed in establishing any detailed positional relations between his units. Berry (*in* Field, 1825) had long ago demonstrated that on the south coast of New South Wales the sandstones that underlie the coal strata rest, at an angle, on older deformed rocks. Little more was done until 1840 when the Rev. W. B. Clarke, who had arrived in Sydney soon after Strzelecki in 1839, joined J. D. Dana in the field at Wollongong. Dana and Clarke observed there a comformable succession from marine fossiliferous sandstones upwards into the coal measures—strata that Strzelecki had assigned to different epochs separated by a revolution. From Dana we have a carefully argued resumé (Dana, 1849, pp. 493–5) of his ideas on the age of the coal. His experience of field relations and the fossil flora and fauna, both vertebrate and invertebrate, led to a conclusion that the whole succession of sandstones, shales and coal belonged to the " upper carboniferous or partly the lower Permian era ". His suggestion of a Permian age had in fact been anticipated by a throwaway remark on the invertebrates that occurred just below the Australian coal in a paper (Koninck, 1846) dealing with fossils from Spitzbergen written by the Belgian chemist and amateur palaeontologist L. G. de Koninck [1809–1887].

Dana's clarity and directness stand in marked contrast with Clarke's responses to the coal problem. Clarke is commonly represented as a staunch advocate of a Carboniferous age for the coal and, until his last years, of the whole succession in what is now called the Sydney Basin. Certainly he maintained, with no little combativeness, such views for many years. I believe they came in part from his failure to recognize differences between the two hemispheres ; he expected an angular break, as in Europe, separating coal measures from Permian and Mesozoic strata.

In the light of Clarke's later position it is strange to find that, soon after Dana's departure, he began to argue an Oolitic (Jurassic) age for the coal. Jervis (1944, p. 428–9) quotes from a letter dated 28 June 1842 from W. S. Macleay [1792–1865] to Clarke stating objections to the case for Oolitic coal expounded anonymously that day in the *Sydney Morning Herald.* It may be noted by the way that Macleay, best remembered as a zoologist, had no mean grasp of geology. He had attended E. D. Clarke's course in mineralogy at Cambridge (Vallance, 1974) and had contributed a discussion on trilobites to Murchison's classic *The Silurian System* (1839). W. B. Clarke's reply (1 July 1842) to Macleay has turned up among some papers preserved by the late J. J. Fletcher and passed to me by Dr. A. B. Walkom. Clarke never actually acknowledged authorship of the newspaper article but the reply leaves no doubt that he was responsible and further that he was not then the devoted adherent of William Smith he later claimed to be. His justification to Macleay rests chiefly on lithological argument. He was evidently roused by Macleay's queries for he returned to the subject in a long rambling memorandum finished at midnight 5 July 1842. He there persists : " if the Sydney sandstone be *New Red*, where are the red and green marls—and the gypsum ? ". To which Macleay responded with the blunt annotation " not necessary ". One wonders whether the excellent paper by J. B. Jukes [1811–1869] warning geologists of the problems of lithological correlation (Jukes, 1843) arose from the author meeting Clarke in Sydney during 1842.

Both Jukes and Clarke had been students of Sedgwick. Jukes evidently did not share Clarke's Oolitic view for in a letter dated 31 May 1843 among the Clarke Papers at the Mitchell Library, Sedgwick tells Clarke that Jukes regarded the Australian coal as stratigraphically lower than that of England. In 1845 H.M.S. *Fly* with Jukes aboard returned to Sydney and during that visit he and Clarke went to the Illawarra district. By then Clarke had gone Carboniferous but Jukes's letter of 1 February 1846 (Jervis, 1944, pp. 381–2) throws light on another aspect of the problem. Jukes there argues to Clarke that what the latter called the Wyanamatta beds must lie above the Sydney or Hawkesbury sandstone and not below as he said Clarke had represented. It must be added, however, that the other side of this correspondence has not been examined. Jukes may have misunderstood his host, for a sketch section accompanying the memo of 5 July 1842 has what Clarke then termed ' Brownlow beds ' clearly above the ' Hawkesbury beds '.

In his important review of Australian geology Jukes (1850) adopted a conservative view as to geological age that agrees generally with that argued by Clarke after 1847. That year saw the publication of M'Coy's palaeontological researches on the collections sent by Clarke to Sedgwick (M'Coy, 1847). By a delicious irony, M'Coy took the Oolitic position abandoned by Clarke. He had been roused by Macleay's criticism in 1842, now Clarke was really stirred. His unfortunate propensity to defend himself with more enthusiasm than prudence became all too evident. For instance, he soon set about proving his Carboniferous model by an attack on Strzelecki's claim that *Lepidodendron* did not occur in the Australian coalfields (Clarke, 1848*a*). Clarke must have seen this fossil plant as required by his case ; the localities he adduced by way of contradicting Strzelecki do not, in fact, belong to the coal areas.

The accidents of the gold discoveries in eastern Australia during and after 1851 deflected geological attention for a time to rocks older than the coal measures. Not until about 1860 did coal problems reassert themselves and by then M'Coy had come to Melbourne as a professor in the new university, a post he combined with that of palaeontologist to Selwyn's geological survey. Fifteen years later there was no general agreement. If we examine Brough Smyth's map of Australia or his explanatory notes (Smyth, 1876) we find a category *Carbonaceous* placed below the Cretaceous ; together they constitute the Mesozoic. Of the strata in the Sydney Basin everything from the coal upwards is lumped in the Carbonaceous. The argument outlined by Smyth (1876) is really that of M'Coy. It was, in fact, as thoroughly determined by European prejudice as was Clarke's. The Australian coal could not be of Palaeozoic age, M'Coy (1867) argued, because certain distinctive (northern) Carboniferous coal plants were not found here. He did, however, admit that fossil fish in the local succession had a Permian or Triassic character, thus taking a point first developed by Dana. And it is only reasonable to admit the validity of his argument that plants from the higher units of the Sydney Basin were of Mesozoic types. In that work of 1867 M'Coy claimed credit for suggesting the existence of Triassic and Permian rocks in Australia—in places remote from the Sydney Basin. Unfortunately knowledge of Permian and Triassic successions in Europe was little help in Australia.

Clarke's side of the argument may be followed through the four editions of his *Sedimentary Formations*, issued between 1867 and 1878 (Vallance, 1969). He ought to be granted the final advantage in the debate. For all the hastiness of his field-work, Clarke at least did examine the succession as it occurs. Indeed the order of strata outlined by Clarke (1866) is now generally recognized if in somewhat different terms. Those like William Keene [1798–1872] and Richard Daintree [1832–1878] who took the trouble to visit the rocks agreed with Clarke. In his important work on Queensland, Daintree (1872) points to the occurrence there of both late Palaeozoic (Carboniferous) coal measures and others of Mesozoic (Carbonaceous) age. Charles Gould [1834–1893], government geologist in Tasmania 1859–69, followed Clarke (Johnston, 1888) but posterity has favoured a Mesozoic attribution for most of the Tasmanian coal. Final sorting of the coal problem in New South Wales came with the advent of men like C. S. Wilkinson and T. W. Edgeworth David, careful field geologists less bound to the primacy of European experience than Clarke and M'Coy and so better able to develop the sort of independence expressed in the work of the Geological Survey of India whence came the germ of the Gondwana concept.

Before leaving the matter of coal we should look at the record in other parts of the continent, if only to see how slight was the rapport between the separate colonies. In both Western and South Australia the need for coal arose before discovery. Moore (1884, p. 376) notes the discovery by J. A. L. Preiss [1811–1883] in 1839 of a fossil 'encrinite' in the west. This was seen as indicating the presence of a Transition or Secondary formation and prompted the government to offer a reward to the first discoverer of workable coal. A few years later, surgeon Joseph Harris in an addendum to remarks by J. W.

Gregory [1815 ?–1850]* on iron ore found near the Swan River argued that as coal and iron usually occurred together in England, coal was to be expected in Western Australia (Gregory and Harris, 1843). Gregory's brother Augustus Charles [1819–1905] in fact found coal at the Irwin River in 1846 (Gregory and Gregory, 1884, p. 8).

The earlier work of the Gregory brothers, like that in eastern Australia some twenty years before, was almost entirely lithological. New Red Sandstone and the like appear, as they do in the reports of F. von Sommer (1848, 1849*a*, 1849*b*) who was employed briefly by the government as a mineralogist. J. W. Gregory (1849), however, notes the presence of fossils in what he calls upper carboniferous limestone at Gingin (was it the Chalk ?) and F. T. Gregory (1861) also reports fossils ; indeed, he is credited with the first clear evidence of Mesozoic rocks in Australia (Moore, 1870). Sommer (1849*c*) offers an idiosyncratic classification of Australian rocks. Perhaps its main interest lies in the (unsupported) claims that Silurian and Devonian rocks exist in South Australia and Triassic rocks in the west. All these pioneers united in regarding the coal as Carboniferous but supply rather than age was the main problem in Western Australia. With the removal of A. C. and, later, F. T. Gregory to the eastern colonies geological activity almost ceased in the west. Not until 1870, with the arrival of the Rev. C. G. Nicolay [1815–1897] and the appointment of H. Y. L. Brown [1844–1928] as government geologist on a two-year contract, did geology begin to revive.

In southern Australia the discovery of order began with the recognition of the Tertiary age of strata along the lower reaches of the River Murray (Sturt, 1833). A more detailed account of Tertiary geology may be found in the work of the Rev. J. E. (T.) Woods [1832–1889] and others (e.g. Woods, 1862). Limestones from about St. Vincent's Gulf, Sturt referred to the Primitive Transition or Transition. Nothing of intermediate age had been recognized by the time Finniss (1843) announced that no secondary rocks existed in South Australia. But as mining activity grew and began to consume the local supply of wood at an alarming rate the need to find other fuels became urgent. Dutton (1846, pp. 311–3) quotes the opinion of C. D. E. Fortnum [1820–1899] on the possibility of finding coal in the colony. Fortnum, remembered as a benefactor at Oxford, spent the years 1840–5 as a settler in South Australia. The remarks and ideal section attributed to him by Dutton are the work of a person with a deal of geological commonsense. On the assumption that any coal would be of Carboniferous age he pointed to places where such strata might exist below the surface. Fortnum's advice to test his model by drilling was not followed but in 1848 surface exploration for coal began on a subscription basis. Dr. G. H. Bruhn of Dresden (Lodewyckx, 1932, pp. 77–8) had come to Adelaide with a party of German miners and, claiming experience of prospecting for coal, offered his services to colonial proprietors. Although disappointed at the response he nonetheless essayed a search. One wonders about his experience for the report (Bruhn, 1849) might suggest that coal geology in Germany had not yet shed Wernerism.

It is interesting to contrast Bruhn with a contemporary German coal expert, Friedrich Odernheimer [1808–1885], employed 1853–6 as mineral surveyor in the Newcastle coalfield by the Australian Agricultural Company. Odernheimer's reports, privately printed for the company, contain much information of value.

* Joshua William Gregory, at the age of 14 arrived with his family at the Swan River in 1829 ; he died there 21 September 1850 [information kindly supplied by the Battye Library, Perth]. By some he has been confused with the unrelated John Walter Gregory [1864–1932], Professor of Geology in the University of Melbourne 1899–1904. The Gregory brothers must be accounted pioneers of geology in the west ; J. W. and F. T. Gregory (1847) prepared the first geological map of Western Australia.

Admittedly there is not much discussion of stratigraphical detail, apart from that relating to coal seams, but no doubt the employers made their requirements clear. Odernheimer was no mere prospector. He came to Australia on the strength of highly-regarded consulting work in Scotland ; his early training included studies at Göttingen, Heidelberg and Clausthal. Of Bruhn's career I know nothing except that after his failure in South Australia success came with a reward of £500 for his part in the discovery of gold in Victoria. But the 'physician Dr. G. H. Bruhn' had left the country before the first part of his prize was paid in 1855. The short account he wrote of Australia (Bruhn, 1855) does little for his reputation as a geologist. On the other hand those that followed him in South Australia during the years to 1875 met with no more success in finding coal. Apart from the Tertiary rocks, the discovery of order in South Australia is an achievement of the last century.

Order Among Older Palaeozoic Rocks

No doubt R. I. Murchison [1792–1871], whose researches in Wales led to his definition of the Silurian System in 1835, was gratified that another Peninsular War veteran reported evidence of Silurian rocks in the Murrumbidgee region of New South Wales so promptly (Mitchell, 1838). He was not to know that graceless posterity deems them Devonian. Verneuil (1840) and Chevalier (1844) also attributed a Silurian age to fossils from the Murrumbidgee hills. Among Strzelecki's collection from the same region Lonsdale (Strzelecki, 1845, p. 296) found material with what he saw as Devonian characters. Two years later de Koninck attributed a Devonian age to a spirifer from Tasmania. These examples and others like Clarke (1848*b*) and Leichhardt (1847, p. 212) mark the first, halting steps towards finding order among those rocks lumped under the Wernerian label *Transition* by earlier investigators. No real progress was made, however, until the discoveries of gold drew attention to the older, folded terrains.

The work of Elie de Beaumont and others in Europe had popularized the notion that mountain ranges sharing a common trend consisted of rocks of similar character and similar age. On that basis it had been argued the mountains of eastern Australia match those of New Zealand and the Pacific coast of South America. Murchison, recently returned from Russia and doubtless influenced by such concepts, offered the suggestion (Murchison, 1844) that the Australian mountains were likewise of a type with the Ural Mountains. More significantly, as the Urals had yielded gold and other precious materials so should the Australian ranges. Two years later Murchison claimed success for his long-range forecasting. W. B. Clarke took up the same line in the *Sydney Morning Herald* for 28 September 1847—and later collided with Murchison in claiming credit for the discoveries of gold in Australia. Neither, in fact, had much substance for his claim but the history of gold discovery as such does not concern me here. It is worth noting, however, that while Clarke was dilating on the Urals, an area of which he had no personal knowledge, there lived in South Australia a man who not only had been there but had written at least two scientific articles dealing with the Urals (Menge, 1826 ; 1842). Such was the parochial state of Australia that no one seems to have thought of seeking his views.

By 1849 mineral discoveries in New South Wales had become sufficiently numerous to move the colonial government to seek the services of a mineral surveyor. Samuel Stutchbury [1798–1859], the man finally appointed, had been in Australia briefly some 25 years before he reached Sydney again late in 1850. He had scarcely commenced his work when the madness of gold erupted and disrupted his survey. Between 1851 and 1855 when the government terminated his appointment he reconnoitred geologically some 80,000 km^2 of the northern part of the colony including part of what since 1859 has been Queensland. Sixteen quarterly reports furnished with maps and sketches to the Colonial

Secretary in Sydney state the results of his work. While Stutchbury was thus engaged Clarke secured a commission to examine other goldfields in the colony. Clarke by collecting and reprinting his reports (Clarke, 1860) avoided the neglect Stutchbury has suffered.

Returning to stratigraphy, we find Stutchbury in his Second Report (dated July 18, 1851) writing of limestones with probable Devonian faunas and in the Fourth Report (January 26, 1852) he presents a case for Devonian rocks at Wellington, N.S.W., on the basis of fossils from limestone and the nature of associated red sandstones. This was the area of the first Australian geological map (Mitchell, 1838) and as far as I am aware Stutchbury offers here the first clear statement on an occurrence of Devonian rocks in this country. Clarke's report of 6 March 1852 from Twofold Bay discloses that he also was then having Devonian thoughts (Clarke, 1860). But whereas Stutchbury went quietly on his way with little opportunity to revise his work, Clarke perhaps erred by writing too much ; from the goldfields reports through the various editions of *Sedimentary Formations* it is possible to trace his difficulties with the Devonian and his susceptibilities to shifts in European opinion.

During the 1860's Clarke's friend Jukes, then attached to the Geological Survey of Ireland, expressed strong doubts as to the validity of the concept of a Devonian System. Briefly, he found difficulty in accepting the Old Red Sandstone and certain slaty rocks of SW England and southern Ireland as belonging to the same system (e.g. Jukes, 1868). The slates he regarded as equivalent to the Irish Carboniferous slates. Clarke, ever sensitive to what he regarded as higher authority, entered a stage of coolness to the Devonian from which he did not finally emerge until reassured by de Koninck in 1876 (Koninck, 1898). Meanwhile, M'Coy was also suffering Devonian problems. Whereas in 1861 he proclaimed pontifically there were no Devonian formations in Australia, a few years later he could announce " with great pleasure " that Devonian limestone existed in Gippsland, Victoria (M'Coy, 1867).

In 1852 the new colony of Victoria, already beset with gold-fever, moved to emulate its northern neighbour and secure the services of a mineral surveyor. Stutchbury was sound ; the man chosen for Victoria was brilliant. A. R. C. Selwyn, as we have seen earlier, came to Australia fresh from the old rocks of North Wales. No more appropriate training-ground could have been found for a man who was to investigate the slates of Victoria. And fortunately, unlike the later Palaeozoic successions in Australia, our older sequences were more approachable by way of European precedent. Starting with little more support than Stutchbury had, Selwyn slowly acquired and trained a staff of assistants (Dunn, 1910). The period 1852–1869 of Selwyn's Geological Survey of Victoria saw the foundation of systematic geological mapping in this country. Hitherto the record had been essentially one of scattered observations. In those 17 years the Victorian survey issued more than 50 district map sheets, a geological map of the colony in eight sheets as well as numerous reports. The papers contributed to the then fashionable international and intercolonial exhibitions provide important reviews of progress (e.g. Selwyn and Ulrich, 1866).

The older Palaeozoic successions in Britain had been argued over since the 1830's when Sedgwick introduced his Cambrian System and Murchison, working stratigraphically higher, his Silurian System. The problem of how the two were related was not helped by Murchison's enthusiasm for his creation. Sedgwick came to believe that Murchison had trespassed on his property. Lapworth's solution of 1879, the erection of an Ordovician System to cover the disputed middle ground, is now generally adopted but does not concern us here. Selwyn the Survey man naturally showed a preference for Murchison and Silurian. The auriferous slates of Victoria on the basis of their graptolite faunas he demonstrated to be of an age with Lower Silurian (Murchison) of North Wales. Here M'Coy's work as palaeontologist to the survey was invaluable. But M'Coy had been for a time Sedgwick's assistant and colleague and it is not surprising to find he called these slates Cambrian.

Establishment of the Cambrian/Lower Silurian System in Australia stands entirely to the credit of Selwyn's survey. Well after 1875 its existence was still limited to Victoria. In New South Wales, Silurian rocks were consistently labelled Upper Silurian. There and elsewhere the sorting of older rocks had to await the attentions of geologists during the past century. Those areas marked as Metamorphic on Brough Smyth's map were all but unknown.

Finale

Geology in Australia to 1875 followed no fine, forward march. Many disputed matters were still unresolved although solutions had begun to emerge. The greatest difficulties concerned those areas in which the European experience gave least guidance and in particular the ordering of the late Palaeozoic to Mesozoic successions. Clarke and M'Coy, who loom so large in this story, strike me as the last of the European school in Australia. Both achieved much of lasting value but their attitudes so firmly shaped by European experience and precedent seem at times to have stultified rather than promoted enquiry. Were they really the founders of Australian geology or, with Selwyn and perhaps Dana, the great forerunners?

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